

must admit the entire correctness of the account given by Dr. Meigs of what he terms child-bed fever, and recommend it to the closest attention on the part of the student and practitioner. Nor do we feel inclined to dispute the general outlines of the author's views in respect to its pathology, or to call in question his directions for its treatment. The objection we make is that, in treating of the subject of child-bed fever, Dr. Meigs has not covered the whole ground—that he has omitted an important form of puerperal fever, marked from its very onset by the most unequivocal indication of adynamia, and in regard to which neither his pathological nor therapeutical views are true; and that, by this omission, they who consult his work for instruction will be misled, by supposing that all cases of child-bed fever consist simply in inflammation of the womb or of its veins, or in inflammation of the peritoneum, and that free and early blood-letting is the only remedy upon which any reliance in their treatment can be placed; and though experience and careful observation will not fail to convince them of the inaccuracy of both propositions, it must, nevertheless, be at the expense of no little mortification, and professional disappointment.

In his pathology of cyanosis neonatorum, Dr. Meigs, we believe, stands alone; and certainly in opposition to all the recorded facts derived from morbid anatomy. In evidence of the correctness of the views advanced by him, he adduces a series of cases to show that cyanosis may be cured by placing the infant in such a position as he supposes will cause the weight of the blood to force down the valve of Botallus upon the open foramen ovale, and thus prevent the direct passage of the blood from the right into the left cavities of the heart. That in many cases of partial asphyxia occurring in children from causes unconnected with organic lesions of the respiratory or circulatory systems, almost immediate relief will be derived by causing the patient to assume the position indicated, is certainly true; it is even true, that in unquestionable cases of true cyanosis, all the prominent symptoms will frequently be very considerably relieved for a time by the same means; this does not prove, however, that the relief in these instances is caused by the closure of the open foramen in the septum between the two auricles of the heart. Such an explanation is a mere hypothesis, unsupported by any direct evidence; and in pathology, one hypothesis cannot be received in proof of the correctness of another.

D. F. C.

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ART. XIX.—*The History, Diagnosis, and Treatment of the Fevers of the United States.* By ELISHA BARTLETT, M.D., Professor of Materia Medica and Medical Jurisprudence in the College of Physicians and Surgeons of the University of the State of New York, etc. etc. Third edition, revised. Philadelphia, Blanchard & Lea, 1852: 8vo. pp. 595.

As a faithful exponent of the present state of public opinion in relation to the history, pathology, etiology, diagnosis, and treatment of the fevers of this country, the work of Dr. Bartlett becomes a valuable if it be not an essential addition to the library of every physician, and we are pleased that the profession have so far endorsed this opinion as to require the issue of a third edition, thus giving the author an opportunity, of which he has industriously availed himself, to incorporate in the different chapters of the treatise whatever additions recent investigations have added to our knowledge of the several subjects discussed.

In treating of the fevers of the United States, Dr. Bartlett has not contented himself with a mere synopsis of the facts and opinions of the leading writers on the subject, but has carefully analyzed and compared their various observations and deductions, and has selected such only as appear to him to present most clearly the indications of accuracy and truth.

Dr. Bartlett strongly asserts the specific character of each of the four forms of fever described by him, namely, the typhoid, the typhus, the periodical, and the yellow fever; each of which, in his opinion, differs essentially from all the others. That ordinarily the several forms of fever alluded to present each well-

marked distinctive characters is unquestionably true, and yet all are aware that cases do occur in which the diagnosis is by no means so easy. Protracted remittents assume, not unfrequently, very much the characteristics of typhoid fever, if they do not in fact occasionally run into the latter. We have repeatedly seen the phenomena of typhoid and typhus fever so intimately blended in the same patient as to render it impossible to determine under which of these forms of febrile disease he was actually labouring, while the more intense grades of bilious remittent fever are often with difficulty distinguished from yellow fever.

However, Dr. Bartlett has certainly the weight of medical authority in favour of the views advanced by him upon this subject. We ourselves *only doubt* the specific difference of the several forms of essential fever, having seen no incontestable evidence by which this difference is established; but we, at the same time, candidly admit, that we have no positive facts to prove that these several forms are mere modifications of one and the same disease. D. F. C.

ART. XX.—*Homœopathy: An Examination of its Doctrines and Evidences.* [Fiske Fund Prize Dissertation.] By WORTHINGTON HOOKER, M. D. New York, 1851: 12mo. pp. 146.

*On the Fallacies of Homœopathy, and the Imperfect Statistical Inquiries on which the Results of that Practice are estimated.* By C. H. F. ROUTH, M. D., M. R. C. S. London, 1852: 8vo. pp. 85.

Dr. HOOKER's essay is essentially the same with the chapter on homœopathy in his work entitled "Physician and Patient," and which has been reviewed in this Journal. It is intended, we presume, even more than that work for popular instruction. It contains much logic and much sarcasm directed against the infinitesimal nonsense and the psoric absurdities of Hahnemann's followers, much more than was necessary for annihilating all that is really vital in these vexatiously minute parasites on the body medical. We cannot congratulate the accomplished author on his success in hunting such small game; the trophies of the chase are hardly worth the bringing home.

The real doctrine contained in the homœopathic system is quite as independent of these fooleries as the latter are of one another. The practice rests upon a tripod, of which the three legs have no physical or metaphysical connection whatever, and which are ludicrously unequal in size, so that the fabric, taken as a whole, tumbles to pieces upon the slightest touch. Not so the homœopathic doctrine. It was conceived and matured before the quackish instincts of Hahnemann felt that it might be made profitable by linking it with a trade, an idea which may very naturally have taken possession of him during his residence among the money-changers of Leipzig, where his brazen image now appropriately adorns the market-place. "Psora" and "globules" were spells invented to lure the silly mob who could not be stirred by any merely scientific charm. Had the formula of *similia similibus* been propounded by an honest physician, it might have excited discord, and, like the Brunonian and other hypotheses, even have arrayed the medical profession in opposing camps. Like these, too, it would more speedily and certainly have achieved whatever good it was capable of. But while by itself it was merely a controllable proposition, it became the scoff and jest of the medical world, by being linked with propositions intrinsically preposterous and absurd.

Hahnemann tells us that homœopathy was the result of his own reflection and observation. It may have been so; but he was certainly not the first who entertained the like notion of medicine. Dr. Hooker quotes from Stahl a passage which explicitly contains the homœopathic doctrine: "The received method in medicine of treating diseases by opposite remedies, that is to say, by medicines which are opposed to the effects they produce, is completely false